

Hoover, Kevin D.

Working Paper

A countercultural methodology: Caldwell's 'Beyond Positivism' at thirty-five

CHOPE Working Paper, No. 2017-13

Provided in Cooperation with:

Center for the History of Political Economy at Duke University

Suggested Citation: Hoover, Kevin D. (2017) : A countercultural methodology: Caldwell's 'Beyond Positivism' at thirty-five, CHOPE Working Paper, No. 2017-13, Duke University, Center for the History of Political Economy (CHOPE), Durham, NC

This Version is available at:

<https://hdl.handle.net/10419/172305>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

**A COUNTERCULTURAL METHODOLOGY:
CALDWELL'S BEYOND POSITIVISM AT THIRTY-FIVE**

BY

KEVIN D. HOOVER

CHOPE WORKING PAPER No. 2017-13

AUGUST 8, 2017



CENTER FOR THE
HISTORY OF POLITICAL ECONOMY
AT DUKE UNIVERSITY

**A Countercultural Methodology:
Caldwell's *Beyond Positivism* at Thirty-five***

Kevin D. Hoover
Department of Economics
Department of Philosophy
Duke University

*This paper was originally written for presentation at a symposium on Caldwell's *Beyond Positivism* at the History of Economics Society Annual Meeting at the University of Toronto, 24 June 2017.

8 August 2017

Abstract

“A Countercultural Methodology: Caldwell’s *Beyond Positivism* at Thirty-five”

Caldwell’s *Beyond Positivism* was a key publication that helped to precipitate the consolidation of the methodology of economics into a distinct subfield within economics. Reconsidering it after thirty-five years, it is striking for its *anti-naturalism* (i.e., its lack of deference to the actual practices of economics) or, perhaps, for its *meta-naturalism* (displayed in its excessive deference to the philosophy of science) and for its defense of pluralism. It offers pluralism as an unsuccessful defense against dogmatism. Against Caldwell’s pluralism, dogmatism is better opposed by a commitment of fallibilism and scientific humility. Caldwell’s defense of Austrian methodology is taken as a case study to illustrate and investigate his key themes and the issues that they raise.

Keywords: Bruce Caldwell, economic methodology, positivism, naturalism, Austrian economics

JEL Codes: B41, B40, B53

A Countercultural Methodology: Caldwell’s *Beyond Positivism* at Thirty-five

*Where have all the flowers gone?
Long time passing
Where have all the flowers gone?
Long time ago.*
PETE SEEGER

I

This symposium falls on the thirty-fifth anniversary of the publication of Bruce Caldwell’s *Beyond Positivism* (1982). Thirty-five years is an odd anniversary to be marking. We routinely track one, five, ten, twenty, twenty-five and fifty years – but thirty-five? Is it right time for a celebration? Not conventionally. So, instead, let me use it as a time for reconsideration and reflection. I come here, not to praise Caldwell, but to take him seriously. To prepare for the symposium, I reread the first edition of *Beyond Positivism*. It was an attempt to recapture the reactions that I had to the book when it was first published. Of course, “you can never step in the same river twice,” “the past is another country,” and “you can never go home again.” My reaction in 2017 cannot help but be conditioned on my own experience and intellectual development since 1982. The same applies to Caldwell: *Beyond Positivism* is juvenilia, and he was an intellectual juvenile when he first wrote it, and when I read it the for the first time. Now that we are graying and, in my case, balding, it is OK to reconsider.

II

Beyond Positivism was published during my first year of graduate school. I stumbled across it simultaneously with Mark Blaug’s *The Methodology of Economics* (1980), which had been published two years before, while trolling the economics books in Blackwell’s Bookshop in Oxford. In retrospect, it is an important book. Economic methodology had, in some sense, existed as long as there had been economists; yet the books of Blaug and Caldwell, as well as Larry Boland’s *The Foundation of Economic Method* (1982), represented a conjuncture or a

turning point: it was with their publication that economic methodologists began to think of themselves as a community that ultimately led to the usual trappings of a field – journals, societies, regular conferences, and so forth.

I had been a philosophy major at William and Mary (also Caldwell’s *alma mater*), but I had never had a course in the philosophy of science and had never thought at all deeply about the special philosophical problems of economics. Thus, *Beyond Positivism* and Blaug’s *Methodology* were a revelation. *Beyond Positivism* is literally where I first encountered the philosophy of science and economic methodology. It was highly readable, very useful, and offered me exceptionally good guidance on what I should read and study. If I know anything at all on these topics, it started there. On the other side, if I have wasted my time on philosophy and methodology, Caldwell also played his part, even if he is not wholly to blame. So, sincerely or ironically, I owe Caldwell!

Casting myself back to 1982, I do recall *Beyond Positivism* left me with an uneasy feeling that I could not adequately articulate. On re-reading the book, I again recognize that sense of unease; yet, I no longer find it hard to articulate. Rather it is now a clear disagreement.

III

There are many details on which I find myself disagreeing with Caldwell, the younger. Rather than quibble, I will concentrate on two larger points that turn out to be related. The first, *naturalism*, the idea that philosophy of science needs to be grounded in the actual practice of science and – in some versions – should eschew prescription, and be deferential to the practices and methodological norms of science, has been a dominant perspective in the philosophy of science over, say, the past forty years. Despite an extensive discussion of the works of Thomas Kuhn, Imré Lakatos, and Paul Feyerabend – important fonts of naturalism – *Beyond Positivism* is

an anti-naturalist work. There is almost no attention to the actual substantive content or practice of economics. The little attention that is paid – for example, in discussions of the rationality postulate – is second-hand, filtered through discussions of explicitly methodological works.

Perhaps it is wrong to characterize *Beyond Positivism* as anti-naturalistic. Perhaps it would be more accurate to say that it is *meta-naturalistic*. The book is not a contribution primarily to substantive debates in either economics or philosophy. Rather it is the *history* of a engagement of economic methodology with a specific thread in the philosophy of science – one that runs roughly from post-World War I Vienna through to the early 1980s.

The book offers surprisingly little connection to the longer history of the engagement of economics with philosophy or with economic methodology. Smith and Mill, for example, were important philosophers and implicitly or explicitly had views on method. Even Marshall and John Neville Keynes had philosophical credentials. Of course, although, one must choose a focus, the choice of focus has consequences. To take one example: Caldwell offers a conventional and, to my mind, deeply misleading account of Milton Friedman’s methodology (characterized as an anti-realist instrumentalism). Friedman, I believe, cannot be understood properly outside of the context of Marshall’s views on methodology, which he imported wholesale and explicitly; and, when understood this way, Friedman is a causal realist and not an instrumentalist (Hoover 2009). Careful attention to the substance of Friedman’s economics rather than to a decontextualized reading of his famous 1953 essay would have pointed straight to Marshall.

As is typical of naturalistic philosophy of science, meta-naturalism is deferential to its target. But here the target is the philosophy of science itself, and Caldwell reports the arguments and the debates and, though he sometimes takes sides, he does not attempt to originate or

advance arguments on the substance. In his discussion of Friedman, a little naturalism then would have been fruitful. But the meta-naturalist pays meta-deference to philosophy rather than economics. Caldwell claims, for example, that Friedman conflates indirect testability of propositions with his dismissal of the need for realistic assumptions, and he remarks, “only the former finds any support in the philosophy of science” (Caldwell 1982, p. 177). This is not an argument; it is the report of an argument; and expresses a kind of privileging – at least for certain purposes – of philosophy. Similarly, Caldwell criticizes Samuelson not for the substance of his methodological views, but for their apparent ignorance of recent developments in the philosophy of science (p. 194).¹

The contrast with Blaug is sharp: Blaug (1980) sets out to say how economists explain or should explain. His object is explicitly prescriptive. Caldwell sets out to explore how accurately economic methodologists have read some parts of the philosophy of science.

IV

Caldwell may well accept the main lines of the points that I have made so far, but simply reply that his goal in writing the book and my preferred goals are different. And that would be a fair response. I am sure, however, that he will object to the points that I wish to make next, as failing to represent his intent in writing *Beyond Positivism*. And that too would be a fair response in its way, because I doubt that in 1982 Caldwell would have wanted to embrace what I see as the consequences of his position, and I am sure that in 2017 he would repudiate them. My point, however, is not whether then or now he would have advocated certain positions that I will

¹ I am reminded that when I was studying music theory, I both had to do exercises in four-part harmony and analysis of musical compositions. When I pointed out to my teacher that Bach routinely violated the rules of voice leading on which she insisted, her response was, “yes, but he’s Bach.” Yes, but he’s Samuelson.

criticize; rather it is that, whether he intends it or not, the positions that he explicitly advocates imply these undesirable positions or, at least, offer no defense against them.

The second point on which I want to focus is Caldwell’s pluralism, which can be neatly summed up as

The policy of letting a hundred flowers bloom and a hundred schools of thought contend is designed to promote the flourishing of the arts and the progress of science.

Of course, that quotation is not from Caldwell; it is from Mao Zedong (1957). Here is Caldwell’s version of a hundred flowers:

the emergence of a single methodology would be most unfortunate, for it would herald the dogmatic straightjacketing of the scientific process in economics. [1982, p. 216]

Avoiding dogmatism is throughout the book held to be the highest value, and its only justification is held to be its utility in promoting scientific progress; yet there is no meaningful analysis of what constitutes scientific progress. Indeed, Caldwell constantly throws cold water on the philosophical attempts to address that question, holding them to be insoluble:

one of the fundamental critical tasks of the methodologist is to repeatedly point out the futility of . . . a search [for an optimal method], while at the same time emphasizing that a place for criticism still exists. [p. 246]

In the end, Caldwell provides us no good understanding of what would constitute a basis on which criticism could properly function, except within the narrow confines of particular programs, and certainly not a basis that addresses any common standard of scientific progress.

I refer to the position of *Beyond Positivism* as “countercultural methodology,” because Caldwell and I belong to the Woodstock generation, and I see *Beyond Positivism* as a reflection of the *Zeitgeist* of the 1970s. Caldwell is a genuinely nice guy. He is self-deprecating, referring to his book as “the scribblings of a philosophical dilettante” (Caldwell 1982, p. 93). His easy-going tolerance is reflected in his gentle libertarianism and his horror of dogmatism – great qualities in my friend, but not, I think, the foundations of a workable methodology. I can recall

exactly when I first encountered the now common trope, “That’s *so* judgmental!” It was in 1979 and spoken by a coworker of my own generation at the Federal Reserve Bank of San Francisco (California, of course!). Naturally, the presupposition is that judgment is, in itself, a bad thing. Typically American (or, perhaps, even more typically Californian), my coworker did not feel in the slightest the irony of passing an unqualified moral judgment on those who judge lesser lapses.

Let me illustrate what worries me with the case of Austrian economics. I do not take this case because I wish to push any particular point of view for or against Austrian economics but because it is a case that Caldwell lays out particularly clearly in *Beyond Positivism*. Skipping some of the details, he notes that various “mainstream” methodologies have dismissed Austrians as dogmatic and reactionary, in part because they are held to be anti-empiricist. So, now the anti-judgmentalism weighs in with an unapologetic *tu quoque* fallacy: “to dismiss the entirety of the Austrian thought as dogmatic and reactionary. . . is itself dogmatic and, at its core, anti-scientific” (Caldwell 1982, p. 119).

What specifically is the problem with the anti-Austrian judgment. At this point it is not clear, and Caldwell provides no analysis of what constitutes dogmatism or why it is bad. Mises, we are told, has answers for his critics. That, however, is hardly enough; the issues is surely whether those answers are adequate. Addressing adequacy presupposes standards or criteria. Caldwell, however, undermines the application of any external standards. Critics, he observe, “fail to take into account that the epistemological foundations of the Austrians and neoclassicals differ” (Caldwell 1982, p. 119). And this is not just a plea for trying to obtain a rich, honest, and empathetic understanding of your opponent’s point of view, which is always helpful in making strong criticism. Rather, Caldwell asserts

the fact remains that a methodological critique of one system (no matter how perverse that system’s tenets may seem) based wholly on the precepts of its rivals (no matter how familiar those precepts may be) establishes nothing. [p. 124; see also p. 165]

Really? If the system’s tenets really are perverse, and do not merely seem to be so, then it would establish a great deal to find that out. But Caldwell points us away from such inquiries as fonts of dogmatism.

While Caldwell mentions the possibility of external criticism, he provides no account of it. When he addresses the question of how Austrian views could be criticized, he implies that external criticism is possible, although criticisms are “*often from within their own frameworks*” (Caldwell 1982, p. 129; his emphasis). Often is not always, but he defines a “a nondogmatic critique” to be “one that does not originate from within the categories of a rival system” (p. 129). Unwinding the double-negative, this implies that a dogmatic critique is one that does originate from within the categories of a rival system.

Caldwell, I think, wants to convey a vision of intellectual tolerance. The term *pluralism* is chosen artfully. It is the intellectual equivalent of multiculturalism, and bespeaks an aversion to power and domination. This is, however, not the vision that his analysis conveys to me. Rather in much the same way that our students often want us to respect their opinions simply because those opinions are theirs, I see this as a vision in which we are asked to accept the internal categories of any school, program, or intellectual group (or, as he puts it, “any system”) for no better reason than that they are the categories that define the system and that the overarching imperative is that we respect each other’s system “no matter how perverse that system’s tenets may seem.” What is more, it is a vision that eschews as illegitimate the kind of inquiry that might resolve whether the tenets merely seem perverse or actually are perverse. Perhaps Caldwell would deny that he has ruled out such inquiry; but, at the least, I cannot see that he has offered us any guidance about how we would conduct one.

Another feature of Caldwell’s analysis is that it requires that we divide thinkers into groups or systems in some well-defined way. For the argument hinges on a clear distinction between being inside and outside of a particular framework. This runs the risk of reducing groups to stereotypes. Naturally, Caldwell, who is a world-class authority on the Austrian school, is the last person that one would expect to reduce Austrians to a homogeneous group. He even notes at various places some of the differences among Austrians (e.g., Caldwell 1982, p. 137, fn. 45). Be that as it may, the argument depends on placing some people on the inside of one conceptual framework and some on the inside of another. It is rather like the currently popular cultural practice of pigeon-holing everyone into specific racial, ethnic, or national categories and resisting any “cultural appropriation” by anyone else. This is, I think, an unfortunate and reductive practice that has trouble acknowledging that people within the approved categories are varied individuals and are constantly biologically, emotionally, socially, and politically falling out of their neat boxes, and that culture has mainly evolved through social exchange and synthesis, and that the only stable relevant category is “human” (and animal rights advocates sometimes challenge even that).

The division of our positions in economics, economic methodology, and philosophy into distinct schools or systems is similarly problematic. Clearly, as Caldwell acknowledges, not every Austrian is a Misesian and many Austrians, like members of every school, refuse to be pigeon-holed into neat boxes and continue to travel and trade across the apparent borders of the schools. I am not, it should be said, maintaining that grouping thinkers into schools or systems is useless or misleading. It is, on the contrary, frequently helpful and insightful. I am asserting, however, that Caldwell’s analysis relies on the implicit assumption that such groups are sharply defined and have impervious boundaries.

This brings us back to Caldwell’s horror of dogmatism. What is wrong with dogmatism? Why not be dogmatic? I do not think that Caldwell gives us any good account. He says that it is inimical to the progress of science, but he gives us no good account of that progress, and indeed undercuts the idea that there could be a good account. In the end, the horror of dogmatism comes down to little more than aesthetic distaste, no different from my coworker’s “That’s *so* judgmental!”

As my philosophical touchstone, Charles S. Peirce, pointed out in 1877, to someone satisfied with dogmatic belief, there is no argument against dogmatism (paras. 377-78). But dogmatic systems are rarely thorough enough to address every human concern. Thus, with respect to many of the ordinary problems of life, our dogmas simply provide no answers one way or the other and, in the end, we end up appealing to commonsensical notions of truth and fact. Once we have such notions in any aspect of our lives, it is possible to generalize them, even to the areas that dogma does address. And once the question of the truth or the facts becomes important to us and we accept that the evidence that resolves our question lies outside of us, then dogmatism begins to appear to be an ineffective way of getting to the truth or the facts. The decisions to value truth and to take a reality external to any particular individual as evidential for truth are choices. If others do not share our values or view of the role of evidence, it may cause us many difficulties, but we have no logically compelling way to show them that they are wrong.

If we do value truth, reality, and evidence, however, we can do a better job at securing the ends that I believe motivate Caldwell’s pluralism. Truth is singular, not plural. That implies, of course, that if we actually disagree, while we may both be wrong, we cannot both be right. The adverb “actually” is critical. One reason to try to make the empathetic transposition of our thinking into another’s system is that it may help us to see which disagreements are genuine and

which are either simply different modes of expression or represent different admissible perspectives (New York looks different from an airliner than from 5th Avenue, but there is no incompatibility). But sometimes we really do disagree. Austrians and neoclassical economists do not, for example, live in isolated intellectual communities - no matter how different their epistemologies. In fact, they not infrequently make substantive claims about how things work in the economy that contradict each other. While their frameworks may seem incommensurable, if they are able to agree or disagree on concrete matters, they are not really incommensurable after all. Surely, it is because their claims *are* commensurable that they can generate any intellectual passion about their differences. We rarely see bitter disputes between stamp collectors and gardeners.

Caldwell laudably opposes dogmatism, but he misunderstands the roots of dogmatism. If we are free to retreat into our methodological or epistemological bunkers, then we have absolutely no reason not to be dogmatic. Dogmatism, he writes, “does not derive from methodological pluralism, but from its opposite” (Caldwell 1982, p. 251). Yet, he concedes that that “methodological pluralism may contain the seeds of dogmatism.” And on that I can wholly agree: those seeds are easy to germinate and make very hardy plants. Caldwell has, as I see it, misdiagnosed the problem, and pluralism offers no firewall against dogmatism. In fact, it licenses it. The real firewall is *humility*. Caldwell calls himself a fallibilist. Fallibilism is an expression of humility: I could be wrong. But one could not be wrong, unless one could also have been right. Fallibilism presupposes that there is one truth, but it also presupposes that one cannot be certain that one has obtained it. It is not pluralism that protects against dogmatism; it is open-mindedness and inquiry in good faith. Pluralism and open-mindedness are, by no means, the same thing.

Caldwell’s *bête noir* is the idea of a unique method of scientific inquiry. In one sense, this strikes me as a red-herring; for, from the point of view, of the actual practice of science, no matter how single-mindedly any philosopher of science has advocated a “method,” no such method has ever been an adequate guide to day-to-day practice: actual sciences, including economics, are too complex and too contextual to stop with coarse guidance. I do not believe that the logical positivists or Popper or Friedman or Samuelson were confused about this point. But at another sense, I am arguing that there *is* a single method: seek the truth, be humble, and, as Peirce says, follow the first rule of reason: “Do not block the way of inquiry” (Peirce 1899, para. 135). As a fallibilist, Caldwell may subscribe to that method; but, of course, it omits the real substance of particular disputes. And that is where I differ from *Beyond Positivism*: it advocates criticism and pluralism; but pluralism provides no basis for criticism; instead, we must find out when our disputes – whether at a substantive or methodological level – are genuine and find the common ground on which to resolve them without retreating into our methodological or epistemological cocoons. If we do that, we might sometimes make progress; if we don’t, we certainly won’t.

References

- Blaug, Mark. (1980) *The Methodology of Economics: Or How Economists Explain*. Cambridge: Cambridge University Press.
- Boland, Lawrence. (1982) *The Foundations of Economic Method*. London: Allen & Unwin.
- Caldwell, Bruce J. (1982) *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: Allen & Unwin.
- Hoover, Kevin D. (2009) “Milton Friedman’s Stance: The Methodology of Causal Realism,” in Uskali Mäki, editor, *The Methodology of Positive Economics: Milton Friedman’s Essay Fifty Years Later*. Cambridge: Cambridge University Press, pp. 303-320.

Mao Tse-tung. (1957) “On the Correct Handling of Contradictions Among the People,” [Speech at the Eleventh Session (Enlarged) of the Supreme State Conference. Comrade Mao Tsetung went over the verbatim record and made certain additions before its publication in the *People's Daily* on June 19, 1957], *Selected Works of Mao Tse-tung*, available online at https://www.marxists.org/reference/archive/mao/selected-works/volume-5/mswv5_58.htm

Peirce, Charles S. (1877) “The Fixation of Belief,” in Charles Hartshorne and Paul Weiss, editors. *The Collected Papers of Charles Sanders Peirce*, vol. 5, *Pragmatism and Pragmaticism*. Harvard: Harvard University Press, pp. 223-247.

Peirce, Charles S. (1877) “The First Rule of Reason,” in Charles Hartshorne and Paul Weiss, editors. *The Collected Papers of Charles Sanders Peirce*, vol. 1, *Principles of Philosophy*. Harvard: Harvard University Press, pp. 56-58.